The book is clearly printed and illustrated, but would be improved if the numbers of the diagrams were given and referred to in the text. Where more than one diagram is found on the same page, it is not always evident to which diagram the lettering refers. The phrase "addition of interest" might also, with advantage, be changed to some other, less suggestive of commercial arithmetic. These, however, are small defects which can easily be corrected in a subsequent edition. The merit of the book is not in *what* it teaches, but in *how* it teaches; and not the least valuable part of it will be found in the introductory remarks addressed to the teachers.

PHILIP MAGNUS.

THE PYRENEES.

Les Pyrénées. Par Eugène Trutat. (Bibliothèque Scientifique Contemporaine.) (Paris: J. B. Baillière et Fils, 1894.)

IN this volume Dr. E. Trutat gives a sketch, as the full title states, of the mountains, glaciers, mineral springs, atmospheric phenomena, flora, fauna, and man in the Pyrenees, illustrated by woodcuts and diagrams, together with two small maps. The mountains differ from the Alps in their greater simplicity of structure, for they form "the most perfect type of a regular chain." Like the Alps, this consists of an axis of crystalline rocks, granites, gneisses, and schists, flanked on both sides by deposits comparatively unaltered. But there is one important difference: in the Alps, systems anterior to the Carboniferous are only recognised in the extreme east; in the larger part of the chain, rocks of that or of a later age rest on crystalline schists, which must be very ancient. But in the Pyrenees schists truly crystalline are succeeded by great stratified masses which have been much less markedly changed. The most ancient of these are assigned to the Cambrian, though as yet fossils either have not been found, or are too ill-preserved to afford any certain evidences of age. It seems, however, clear that they are older than the Silurian system, for the different members of this can be identified in several places by their characteristic fossils. The Devonian system is well developed, and followed by limestones (marine), conglomerates, and slaty rocks of the Carboniferous period. The occurrence of Permian rocks is considered by Dr. Trutat to be doubtful. Trias, of the Lorraine type, is found, followed by representatives of the various systems in orderly succession up to the Neocomian. Between this and the Cretaceous is a break, then the sequence continues till after the Nummulitic age. Then, as in the Alps, began the great series of movement, of what the present chain is the outcome. Masses of eruptive rock are connected with these disturbances. The enormous beds of conglomerate, called the *Poudingues de Palassou*, which sometimes surpass 1000 metres in thickness, recall the Alpine *nagelfluhe*. Strata partly marine, partly freshwater, represent the Miocene and the Pliocene; the Quaternary deposits presenting a general resemblance to those of the Alps.

The glaciers of the Pyrenees at the present day are comparatively small, the length of the largest not exceeding about 4300 metres, while that of the Great Aletsch is 32 kilometres, but their former extent, as in the Alps, was much greater. They filled the valleys, and even debouched on the lowland; that of the valley of Ariège must have been about 70 kilometres long. The glacial deposits have been assigned to two epochs, and Dr. Trutat claims for the earlier a considerable antiquity. In the Ariège he states that they underlie Pliocene marls, and near the plateau of Lannemezan pass under the Miocene deposits of Sansan. The sections which he gives are very rough, and further proofs of these statements, which involve obvious difficulties, are likely to be demanded. The Pyrenees owe their existence, as has been said, to post-Nummulitic disturbances, but they also afford evidence of great movements, both anterior to the Carboniferous and after the Neocomian, the latter apparently being less marked. Movements occurred after the great post-Eocene elevation, but of much less importance than they were in the Alps. The other topics, mentioned on the title-page, receive due notice, and the volume will be found useful, as it gives, in a concise and convenient form, much information about one of the most important mountain chains in Europe.

T. G. B.

OUR BOOK SHELF.

Les Courants Polyphases. Par J. Rodet et Busquet. (Paris: Gauthier-Villars et Fils, 1893.)

TO those desirous of obtaining a general knowledge of the principles used in the calculation of the efficiency and of the dimensions of polyphase motors, &c., this book will be of considerable use. In the first part, the calculation of the dimensions of, and losses in, the conductors conveying the currents are worked out at some considerable length, numerical examples being given. In the other sections the generation of polyphase currents, motors with rotating fields, and transformers are dealt with; in each case the general principles of the machines now in use being described, though no account is given of the details of their construction. There is also a short account of some of the plants for the transmission of energy by polyphase currents which have been installed, with a table summarising the tests and measurements made during the Frankfort Exhibition.

Solutions of the Examples in the Elements of Statics and Dynamics. By S. L. Loney, M.A. (Cambridge: Camb. Univ. Press, 1893.)

MR. LONEY is indebted to a friend for these solutions, and also for the revision of the whole of the proof-sheets. We have glanced through many of the examples, and they seem to be fully and clearly worked out on the whole, very little being taken for granted. Students who cannot depend on the presence of a teacher, will find that with a judicious use of this key much may be self-taught.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Sir Henry Howorth and "Geology in Nubibus."

SIR HENRY HOWORTH, in his reply to Dr. Wallace and Mr. LaTouche, concerning the excavating power of ice, remarks that he is "speaking to every man of science, geologist or

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otherwise." Indeed, from the tone of his letter he would appear to be defending modern science against the attacks of certain unscientific persons who hold extreme views on glacial questions. As one who has taken a great interest in this subject for a number of years, I trust that I may be allowed to add a few words to the discussion.

We are required by Sir Henry Howorth to establish two postulates. "(1) That ice can convey thrust for more than a moderate distance. (2) That glaciers, such as we can examine and report upon, are anywhere at this moment doing the excavating work . . ." Dr. Wallace postulates.

In reply to the first, we have the undoubted fact that in hundreds and thousands of instances striated rock surfaces do occur hundreds of miles from existing glaciers. On this point he remarks: "If glaciers travelled further in former days, it was doubtless because glaciers were larger in former days, because they descended longer slopes, and had larger gathering grounds; that is to say, because the country where they grew was more elevated." So the glacial period resulted from elevation, and all glaciated regions conveniently rose together to produce it, and as conveniently sank down again. I was quite unaware that this was the accepted view. We have no proof whatever that the striated slopes down which the old glaciers moved were steeper in glacial times than they are now. Indeed, the proof is all the other way, and we may consider it as proved that at long distances from their sources, and on comparatively level plains, glaciers have moved, and have polished, ground, scratched and *grooved* the rocks over which they passed. The only point about which there may be legitimate discussion concerns the possible extent of the abrasion.

In his mechanics Sir Henry Howorth is, I am afraid, rather unsound. There are really two factors upon which the possibility of motion in a viscous body depends. One is, of course, the slope of the surface over which it passes, and the other is the slope of the upper surface of the viscous body. Fracture and regelation have little to do with the question, for fracture only occurs near the surface, and fracture must not be confounded with *shear*. Sir Henry Howorth makes one statement which seems to account for the conclusions he has come to. It is "a viscous body, unless the viscosity approaches that of a liquid, cannot move by mere hydrostatic pressure." In fact he assumes, without adducing a particle of evidence in support of the assumption, that there is an inferior limit to the stress required to deform *glacier* ice. I always regarded viscosity as something which retarded motion, but did not in any way interfere with the ultimate result. I have personally made mechanical tests of ice, and also of many thousands of samples of steel, iron, copper, brass, tin, &c. All these substances yield elastically and permanently under stress, some of them under very small stresses, but ice is the only one of them that yields continuously from the moment the stress is applied until it is again removed.

It is not, properly speaking, pressure from behind that forces the ice forward. Ice being viscous, every individual particle moves in the direction of least resistance at a rate depending upon the stress and the viscosity. Sir Henry Howorth may term this "Geology in Nubibus," and call it unmechanical; but I would point out to him that I regard the question from the point of view of a *mechanical engineer*, which I am afraid he does not.

During the past summer I had the pleasure of seeing some of the Norway glaciers, and also of crossing the Folgefond snow field. It was interesting to note that although the streams coming from the hills and uplands free from ice were quite clear, those escaping from the glaciers were charged with sediment. In this connection I would call attention to a calculation made by Prof. G. F. Wright, giving the rate of erosion of its bed by the Muir Glacier. From the volume and turbidity of the water he makes the figure one-third of an inch per annum over the whole of the 1200 square miles of area occupied by the glacier. In fact, erosion goes on much more rapidly when the rocks are covered by moving ice than when they are not. Although we may feel absolutely certain, both by fact and reason, that the erosion beneath glaciers when they are moving with relative rapidity is very great, and be as sure as we reasonably can be of most things that such erosion must result in the formation of lake basins, I am afraid that we shall be unable to satisfy Sir Henry Howorth on the point. We cannot remove a glacier, and if there should prove to be a rock basin below measure of its depth, then replace the ice, and measure again, say, in a thousand years. This is the kind of proof the second postulate seems to demand.

R. M. DEELEY.

The "Zoological Record."

IN reference to the letter of Messrs. Pocock and Bather, on the subject of the *Zoological Record*, in NATURE of November 16, I desire to state that the council of this Society (to which the *Zoological Record* at present belongs) quite agree with the above-named gentlemen in their wish to render the *Zoological Record* more complete by combining palæozoology with it. With this view the council some time since addressed the Geological Society, and suggested what in their opinion would be an equitable arrangement for carrying out the plan. This arrangement, however, as will be seen by the copy of the correspondence enclosed herewith, was rejected by the council of the Geological Society. It remains, therefore, for Mr. Bather and such members of the Geological Society as may share his sentiments, to do their best to induce the council of the Geological Society to alter their views upon this question.

I have good authority for stating that the editor of the *Zoological Record* is not really of a different opinion to Messrs. Bather and Pocock on this subject, as would seem to be inferred in their letter. I believe that he only suggested that the palæontologists should start a record for themselves because of the refusal of the Geological Society to co-operate with us in our work.

P. L. SCLATER.

Zoological Society of London,
3, Hanover Square, London, W.

(COPY.)

To the Secretary, the Geological Society, Burlington House, W.

DEAR SIR,—I am instructed by the council of this Society to apply to the council of the Geological Society under the following circumstances:

When the *Zoological Record* (which is now carried on by this Society) was established twenty-eight years ago, it was not considered that palæozoology came within its scope, and the recorders were instructed to notice only such palæontological works as appeared to be "of interest to the student of living forms" in their records. This part of the subject has, however, received a continually increasing amount of attention from the recorders, and the council of this Society, being desirous that palæozoology should in future be treated of in the *Record* as completely as recent zoology, asks the assistance of the Geological Society in carrying out this object.

The Zoological Society bears at present a loss of about £350 per annum, arising from the publication of the *Zoological Record*, and, as the inclusion of palæozoology in an exhaustive manner would materially increase the work of the recorders, and necessitate an addition to their remuneration (which is even at present too small), the council of this Society asks the council of the Geological Society to make a grant of £100 annually towards the expenditure thus incurred.

It is thought by some members of the Council that the Zoological Society, bearing, as it does at present, the whole loss arising from the publication of the *Record*, should not increase its expenditure thereon, and the sum mentioned above, £100, would, it is estimated, be sufficient to relieve this society from the additional expense that the inclusion of palæozoology, in its record of zoological literature, would involve. In return for this assistance, the council of this Society will make every effort to treat the subject of palæozoology exhaustively, and will add to each *Record* a reference to palæozoological memoirs stratigraphically arranged, besides dealing with those memoirs in detailed analysis in the systematic records.

In order that the interests of palæozoology may be more carefully attended to, the council of this Society will undertake to place a nominee of the Geological Society upon the committee of their body appointed every year to supervise the publication of the *Zoological Record*.

Should these proposals meet with acceptance from the Geological Society, the council will further undertake to place one hundred copies of the *Zoological Record* at the disposal of the Geological Society, and, if it be wished, will alter the title of the *Record* to *The Record of Zoological and Palæozoological Literature*, from *The Record of Zoological Literature*.

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